Bellamy Clifton, M.A.; William Turner Thiselton Dyer, C.M.G.; Professor James Alfred Ewing, M.A.; Lazarus Fletcher, M.A.; Walter Holbrook Gaskell, M.D.; Professor Alfred George Greenhill, M.A.; William Huggins, D.C.L.; Professor Charles Lapworth, LL.D.; Major Percy Alexander MacMahon, R.A.; Professor Raphael Meldola, F.C.S.; Professor William Ramsay, Ph.D.; The Lord Walsingham, M.A.; Professor Walter Frank Raphael Weldon, M.A.; Admiral William James Lloyd Wharton, C.B.

The following Papers were read:—

I. "Mathematical Contributions to the Theory of Evolution. On Telegony in Man, &c." By KARL PEARSON, F.R.S., University College, with the assistance of Miss ALICE LEE, Bedford College, London.

II. "On the Magnetic Permeability of Liquid Oxygen and Liquid Air." By J. A. FLEMING, M.A., D.Sc., Professor of Electrical Engineering in University College, London, and JAMES DEWAR, LL.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution.

"Mathematical Contributions to the Theory of Evolution. On Telegony in Man, &c." By KARL PEARSON, F.R.S., University College, with the assistance of Miss ALICE LEE, Bedford College, London. Received August 27,—Read November 26, 1896.

(1) The term telegony has been used to cover cases in which a female A, after mating with a male B, bears to a male C offspring having some resemblance to or some peculiar characteristic of A's first mate B. The instances of telegony usually cited are (i) cases of thoroughbred bitches when covered by a thoroughbred dog, reverting in their litter to half-breds, when they have been previously crossed by dogs of other races. Whether absolutely unimpeachable instances of this can be produced is, perhaps, open to question, but the strong opinion on the subject among dog-fanciers is at least remarkable; (ii) the case of the quagga noted by Darwin (see 'Origin of Species,' 4th edition, p. 193), and still more recently (iii) a noteworthy case of telegony in man cited in the 'British Medical Journal' (see No. 1834, February 22, 1896, p. 462).

In this latter case a very rare male malformation, which occurred in the male B, was found in the son of his widow A, by a second husband C. Here, as in the other cases cited, a question may always be raised as to the possibly unobserved or unknown occurrence of the

characteristic in the ancestry of either A or C, or again as to the chance of the characteristic arising as a congenital sport, quite independently of any heredity. It seems unlikely that the observation of rare and isolated cases of asserted telegony will lead to any very satisfactory conclusions, although a well-directed series of experiments might undoubtedly do so. On the other hand, it is not impossible than an extensive and careful system of family measurements might bring to light something of the nature of a telegonic influence in mankind.

If such a telegonic influence really exists, it may be supposed to act in at least two and, very possibly, more ways.

(*a*) There may be in rare and isolated cases some remarkable change produced in the female by mating with a particular male, or some remarkable retention of the male element.

(*b*) There may be a gradually increasing approximation of the female to the male as cohabitation is continued, or as the female bears more and more offspring to the male.

It is extremely unlikely that any system of family measurements would suffice to bring out evidence bearing on (*a*). On the other hand, a closer correlation between younger children and the father, and a lesser correlation between younger children and the mother, as compared with the correlation between elder children and their parents might, perhaps, indicate a steady influence like (*b*) at work in mankind. Shortly, such measurements might suffice to answer the question as to whether younger children take more after their father and less after their mother than elder children. Without hazarding any physiological explanation as to the mode in which telegonic influence can or does take place, we may still hope to get, at any rate, negative evidence as to a possible steady telegonic influence by an investigation of suitable family measurements.

(2) Unfortunately, the collection of family data is by no means an easy task, and to procure those head-measurements, which, I think, would be most satisfactory for the problem of heredity, would require a large staff of ready assistants, and could only be undertaken on the necessary scale by the action of some scientific society or public body. The data concerning 800 to 900 families which have been recently collected for me deal only with stature, span, and arm-length, which are measurable with more or less accuracy by the untrained observer, and are only suitable for more or less rough appreciations of hereditary influence. The numbers in each family measured were strictly limited, in order to remove the influence of reproductive selection from the determination of the correlation between parents and children, and the result of this limitation has been that comparatively few couples of elder and younger brothers, and of elder and younger sisters are available. They were, indeed, collected in the

first place with a view to the problem of heredity in the direct line, and with no thought of their throwing any light on the problem of telegony. That steady telegonic influence might be deduced from such family data has only recently occurred to me, and I should now hesitate to publish any conclusions on this subject, based on somewhat mixed and sparse returns, did I not consider that it may be a long time before more extensive returns are available, and that the publication of this method of dealing with telegony may induce others to undertake the collection of a wider range of material.

My own 800 family data cards did not provide a sufficiently large number of either brother-brother or sister-sister couples to give a strong hope of a difference between the correlation coefficients sufficiently large as compared with its probable error to base any legitimate conclusion upon. I, therefore, again borrowed from Mr. Galton his 200 family data returns, and from these 1,000 families was able to select 385 brother-brother pairs and 450 sister-sister pairs. In these statistics each individual is only included in one pair, and the difference in age between the elder and younger members of each pair differs very widely from pair to pair. In some cases there may be several years between the ages and several intervening children; in others the members of the pair may be successive children following each other in successive years. In each case all we can say is, that if there be a steady telegonic influence, the relation of the elder member to the parent will weigh down the same scale, and in the final result we ought to find a distinctly greater or less correlation, as the case may be. I think a more serious objection to the data than the variation in the number of years between fraternal pairs is the mixture I have made of data collected at different periods and in somewhat different manners. My own data are drawn, I think, from a wider class of the community than Mr. Galton's. They are not exclusive of his class, but, I think, cover his class, and go somewhat further down in the social scale. They suffice to show that the means and variations change considerably from one social stratum to another, and what is still more important that the Galton-Functions or coefficients of correlation for heredity are far from being constant even within the same race, as we pass from one rank of life to a second. Thus, my means for stature in the case of both fathers and mothers are upwards of $\frac{1}{2}$ in. less than Mr. Galton's, but my means agree fairly well with his results in the case of both sons and daughters. There are also good agreements and somewhat puzzling disagreements not only in the variations, but, above all, in the coefficients of correlation for heredity. I reserve for the present the full discussion of my heredity data, but I wish it to be quite understood that my conclusions in this paper are based, not upon the best possible data, *e.g.*, measurements made on one class of the com-

munity under one system, but upon all the data which, for some time to come, appear likely to be available. These data are neither quantitatively nor qualitatively ideal, but, on the other hand, they must be given a reasonable amount of weight in considering whether, at any rate in the case of one organ—stature,—any steady telegonic influence can be traced in man.

The reduction from the family measurement-cards, the formation of the eight correlation tables, and the calculation of both variation and correlation coefficients have been undertaken by Miss Alice Lee of Bedford College,—a task requiring much labour and persistency. I have independently verified, and in some minor points corrected her calculations, as well as added the probable errors of the constants determined.

(3) The following are the means and standard-deviations with their probable errors for the various groups.

Table I.—Stature of Families in Inches.

Class.	Number.	Mean.	Standard deviation.
Fathers of sons	385	68·5740±0·0878	2·5554±0·0621
Elder sons	,,	69·1494±0·0913	2·6550±0·0645
Younger sons	,,	69·1948±0·0933	2·7128±0·0659
Mothers of sons	,,	63·3078±0·0854	2·4848±0·0604
Fathers of daughters	450	68·3344±0·0878	2·7605±0·0621
Elder daughters	,,	63·9244±0·0823	2·5878±0·0582
Younger daughters	,,	64·2200±0·0794	2·4985±0·0562
Mothers of daughters	,,	63·1794±0·0758	2·3827±0·0536

All the quantities have here been calculated precisely as in my third memoir on the mathematical theory of evolution (see 'Phil. Trans.,' A, vol. 187, pp. 270—271). In this case, however, no child is included twice as a child, and parents are not weighted with their offspring. Thus reproductive selection is not allowed to influence the results.

It will be seen that the probable errors of the means and standard deviations are, as in the former paper, too large to allow of absolutely definite conclusions when those conclusions are not supported by a continuous change of values, or directly verified by the numbers of the earlier memoir. But one or two such conclusions may be drawn, and I will note them before passing to correlation.

(i) The law of sexual interchange referred to in my former paper (p. 274) is confirmed with greater uniformity. Fathers of sons are sensibly less variable than fathers of daughters, and mothers of daughters are sensibly less variable than mothers of sons. In other

words, to judge from stature, the exceptional parent tends to have offspring of the opposite sex.

(ii) Younger sons are taller and more variable than elder sons, and elder sons are taller and more variable than fathers.

This conclusion, although less markedly, appears in the results on pp. 270 and 281, of my former paper. It might be accounted for by:

(*a*) A secular change going on in the stature of the population, and even noticeable in the difference between the stature of younger and elder sons.

(*b*) A further growth of sons, and an ultimate shrinkage, which will leave them at the age of their fathers with the same mean height and variation.

(*c*) Conditions of nurture on the average less favourable, and on the whole less varied in the case of elder than in that of younger children.*

(*d*) Natural selection. The difference between younger and elder sons and between elder sons and fathers represents the selective death rate in man due to causes correlated with stature in the years between youth and manhood, and manhood and age. The difference is thus to be accounted for by a periodic and not a secular change.

Possibly (*a*), (*b*), (*c*), and (*d*), may all contribute to the observed results. It cannot be denied that (*d*) has a special fascination of its own for the student of evolution, but prolonged study of the laws of growth must precede the assertion that we have here, or in any similar case, real evidence of an actual case of natural selection.

(iii) Younger daughters are taller than elder daughters and elder daughters than mothers.

This is in complete agreement with the result for fathers and sons. Further:

Daughters, as a class are far more variable than mothers, but while in the earlier memoir younger daughters were sensibly more variable than elder daughters—and thus exactly corresponded with sons—elder daughters are in this case more variable than younger. I have been unable to find any slip in the tables or calculations, which might account for this divergence. It exceeds considerably the probable error of the observations, and is not in accordance with the general law connecting the variation of parent and offspring evidenced for both sexes in the earlier, and for sons in the present memoir—*e.g.*, the variation—whether it be due to growth-change,

* Mr. Francis Galton suggests this as a possible cause. It has, I think, to be taken in conjunction with a greater amount of parental experiment, not only in the birth, but in the nurture of the elder children.

or to selective death-rate, or to secular evolution—diminishes with age.

(4) The following are the coefficients of correlation (r) and the coefficients of regression (R) for parents and sons:

Table II.—Inheritance of Stature by Sons.

	r.	R.
Father and elder sons	0·4120±0·0264	0·4281
Father and younger sons....	0·4170±0·0262	0·4427
Mother and elder sons	0·4094±0·0265	0·4374
Mother and younger sons ...	0·4111±0·0264	0·4488

If we measure, as seems reasonable, the hereditary influence of parentage by the magnitude of the coefficient of correlation between parent and offspring, then several important conclusions may be drawn from this table.

(i) There is no sensible difference between the influences of the father on younger and on elder sons, and no sensible difference between the influences of the mother on younger and on elder sons.

If we pay attention to such slight differences as exist, there would appear, not to be an increase of paternal and a decrease of maternal influence on younger children, but an extremely slight increase of both. In other words, so far as stature in sons is concerned, judged by correlation: *No steady telegonic influence exists.*

(ii) There is a very slight prepotency of the father over the mother in the case of both younger and elder sons; a prepotency which will be slightly magnified when account is taken of the absolute stature of the two parents.

But the great prepotency of paternal inheritance noticed in the former memoir is not confirmed. The co-efficients of maternal inheritance have been increased by more than 30 per cent. (from 0·293 to 0·410), while those of paternal inheritance (0·396 as compared with 0·414) have remained almost stationary. This result seems to show the want of constancy of the Galton's functions for heredity within the same race. An explanation on the ground that the present statistics embrace a wider range of the community than the earlier, and possibly a more closely correlated class,* fails, at any rate in part, owing to the sensible constancy of the paternal correlation. The main difference of course between the present and the former statistics is the exclusion of the influence of reproductive

* I have pointed out (*loc. cit.*, p. 284) that working and lower middle class families appear to be more closely correlated than those of the upper middle class.

selection, but why should this be expected to influence only the mother? The father of many children remains equally influential, but the mother's relation is weakened when we give weight to the quantity not the relative ages of her children. This is not a steady telegonic influence, but a correlation between fertility and hereditary influence in mothers, which if it could be verified by further observation, would undoubtedly be of high significance. I would accordingly suggest as a possible law of heredity, deserving careful investigation, that: *Hereditary influence in the female varies inversely as fertility.*

In my paper on "Reproductive Selection," ('Roy. Soc. Proc.,' vol. 59, p. 301), I have pointed out the important evolutionary results which flow from a correlation between fertility and any inheritable characteristic. If a law of the above character should be established after further investigation, it is conceivable that it may act as an automatic check on the extreme effects of reproductive selection.

(iii) The above results give us for practicable purposes a quite sufficiently close value of the correlation between parents and sons, when the influence of reproductive selection is excluded. Judging from stature the correlation between sons and parents is very closely given by

$$0{\cdot}41 \pm 0{\cdot}03.$$

The $\frac{1}{3}$, adopted by Mr. Galton, may, I think, safely be increased by 25 per cent., and further, the assumption that collateral heredity is twice as strong as direct heredity must, I hold, be finally discarded, for no determination of the former has given such a high value as 0·82.

(5) Hitherto we have regarded only the coefficients of correlation, and considered them to measure the strength of the hereditary influence, but it must be remembered that the means of elder and younger sons are not the same, and that there is another way of looking at the problem. We may ask: Do younger or elder sons differ most from the stature of their father, and is the order altered in the case of the mother?

If we neglect the influence of sexual selection (see "Contributions to Math. Theory of Evolution," III, pp. 287—8) we have, if h_f and h_m be deviations of father and mother from their means, and M_e and M_y be mean heights of corresponding fraternities of elder and younger sons in inches:

$$M_e = 69{\cdot}1494 + 0{\cdot}4281h_f + 0{\cdot}4374h_m.$$

$$M_y = 69{\cdot}1948 + 0{\cdot}4427h_f + 0{\cdot}4488h_m.$$

Now the ratio of the mean heights of parents is 68·5740 : 63·3078 =

1·0832,* while the ratios of 0·4374 to 0·4281 and 0·4488 to 0·4427, are only 1·0219 and 1·0139 respectively, thus there is still a slight prepotency of paternal influence on stature to be recorded. (See § (4) (ii).)

Confining our attention to the differences in stature for fathers and sons corresponding to all mothers whatsoever, we have, if D_{ef} be the difference in stature between father and corresponding fraternity of elder sons, D_{yf} between father and fraternity of younger sons:

$$D_{ef} = 0{\cdot}5754 - 0{\cdot}5719 h_f.$$

$$D_{yf} = 0{\cdot}6208 - 0{\cdot}5573 h_f.$$

Hence the difference betwen the father and fraternity of younger sons will be greater than the difference between the father and the corresponding fraternity of elder sons unless the father be 3·110 inches less, or 1·059 more than the average. But 3·11 is about 1·2 and 1·059 about 0·415 times the standard deviation of the stature of fathers, or, fraternities of younger sons are nearer in stature to their father than fraternities of elder sons in about 46 per cent. of cases.

Similarly if D_{em}, D_{ym} represent the differences of stature of mothers and fraternities of elder and younger sons respectively, we have in inches

$$D_{em} = 5{\cdot}8416 - 0{\cdot}5626 h_m.$$

$$D_{ym} = 5{\cdot}8870 - 0{\cdot}5512 h_m.$$

Thus fraternities of younger sons are always more divergent than fraternities of elder sons from the stature of their mothers, unless the mother be 3·982 inches less, or 10·53 inches more than the average. These are 1·6 and 4·24 times the standard deviation in stature of mothers; or, only in about 5·5 per cent. of cases are fraternities of younger sons nearer in stature to their mothers than elder sons.

Now, it is difficult to read into these results any evidence for a steady telegonic influence. It is true that the case of younger sons being more like their parents than elder sons occurs in eight times as many cases with the father as with the mother, but the broad fact remains that in more than half the cases, judged by difference of stature, the elder son is more like the father than the younger son. In fact, examined in this way by difference of stature—not an unnatural manner of first approaching the problem—the true closeness of parent and offspring appears to be quite obscured by some secular, or, at any rate, periodic (see § 3) evolution in stature between successive generations—an evolution which even makes itself felt in the interval between younger and elder sons.

* 13/12 = 1·0833; thus these returns again confirm Mr. Galton's selection of this fraction for the sexual ratio for stature.

(6) Turning to the results for daughters, we have the following table for the coefficients of correlation and regression :—

Table III.—Inheritance of Stature by Daughters.

	r.	R.
Fathers and elder daughters.....	0·4829±0·0220	0·4528
Fathers and younger daughters	0·4376±0·0236	0·3961
Mothers and elder daughters	0·3953±0·0250	0·4293
Mothers and younger daughters ...	0·4542±0·0230	0·4763

These results, more numerous than those for sons, are, for reasons which I am unable to explain, much more divergent. We may note the following points :—

(i) There is a sensible difference between the coefficients of correlation for either parents with younger and elder daughters. Thus, the difference of the coefficients for fathers with elder and younger daughters is 0·0453, and the probable error of this only 0·032; while for mothers the corresponding difference is 0·0589, and the probable error of the difference only 0·0328. The difference, however, is in the *opposite* sense. We are thus face to face with an increasing maternal and a decreasing paternal influence on the stature of daughters. In other words, our statistics are entirely opposed to any steady telegonic influence on the stature of daughters. If such a thing were conceivable, we should be confronted with the case of the mother influencing the father, the reverse of telegony.

(ii) The mean correlation of fathers and daughters is very slightly higher than that of mothers and daughters (0·4602 as compared with 0·4247). Thus, to judge by the mean coefficients of correlation, the father is slightly more prepotent than the mother in heredity. The mean coefficients of regression are for fathers 0·4244, and for mothers 0·4528, or in the ratio of 1 : 1·067, but the ratio of the paternal to the maternal stature is 1·083, or this slight prepotency is still preserved if we judge the matter by regression coefficients. Again, we notice an immense increase (0·2841 to 0·4247) in the correlation between mothers and daughters when we compare the present results with those of my earlier memoir. As an explanation of this, I have already suggested the possibility of a law exhibiting a relation between fertility and hereditary influence in mothers (§ 4 (ii)).

(iii) The mean coefficient of correlation in stature between either parent and a daughter may be taken to be—

$$0{\cdot}44 \pm 0{\cdot}02.$$

Thus, it does not differ very widely from the value suggested (0·41) for sons, but is even further removed from the value (0·33) at first determined by Mr. Galton.

The greater correlation between sons and both parents noticed in my first memoir is not borne out by the present statistics; the advantage is now—it is true to a much less extent—with daughters.

On the whole, I am not well satisfied with these results for daughters. I can see no persistent source of error in the method of collecting the observations, nor can I find any mistake in the calculations. I can only trust that more elaborate returns and measurements of other characteristics may some day throw light on what now appear to be anomalies.

(7) Finally, I may just notice what conclusions are to be drawn, if we pay attention to the absolute difference in stature between parents and daughters. Let δ_{em} and δ_{ym} be the differences in stature between elder daughters and mothers, and younger daughters and mothers respectively, then in inches we have for the corresponding arrays:

$$\delta_{em} = 0{\cdot}7450 - 0{\cdot}5707 h_m.$$

$$\delta_{ym} = 1{\cdot}0406 - 0{\cdot}5237 h_m.$$

Thus, arrays of younger daughters differ more from their mothers in stature than arrays of elder daughters, if the mothers be more than 6·29 in. below the mean or more than 1·63 in. above the mean, or if their deviations are not within the limits of about −2·64 and 0·68 times the standard deviation of mothers. This gives us about 74 to to 75 per cent. of elder sisters nearer in stature to their mothers than younger sisters.

If δ_{fe}, δ_{fy} be the stature differences for fathers and daughters, we have:

$$\delta_{fe} = 4{\cdot}4100 - 0{\cdot}5472 h_f.$$

$$\delta_{fy} = 4{\cdot}1144 - 0{\cdot}6039 h_f.$$

Here, so long as the father lies between 5·21 in. less and 7·41 in. more than the average, the array of younger daughters will more nearly approach him in stature than the array of elder daughters. These limits correspond to 1·89 and 2·68 times the standard deviation of fathers. Accordingly, about 96 to 97 per cent. of younger sisters are closer in stature to their fathers than elder sisters. Thus, if we had started the discussion of the problem from a consideration of the relative nearness in stature of daughter to father and mother, we should have found that a great majority of younger sisters were nearer to their fathers than their elder sisters, and a considerable majority of elder sisters nearer to their mother than their younger sisters. We might then have concluded that there were substantial

grounds for inferring the existence of a telegonic influence. But it is clear that if there be anything of the nature either of a periodic or of a secular change in stature going on, then since men are taller than women, any group of younger women will appear closer to their fathers than to their mothers, when compared with a group of elder sisters. Thus, no legitimate argument as to a telegonic influence can be based on such a result. I have purposely considered this method of approaching the problem, because it is the method which first occurred to me, as it probably may do to others. It can very easily, however, lead to our mistaking for a real telegonic influence an effect of periodic or secular evolution, or, indeed, of different conditions of nurture.

(7) In conclusion, we may, I think, sum up the statistics discussed in this paper as follows:—

(i) So far as stature is concerned there is no evidence whatever of a steady telegonic influence of the male upon the female among mankind.

(ii) It is improbable that the coefficients of correlation which measure the strength of heredity between parents and offspring are constant for all classes even of the same race.

For stature in the case of parents and offspring of both sexes, the value 0·42, or say 3/7, may be taken as a fair working value, until more comprehensive measurements are made. This makes hereditary influence in the direct line stronger than has hitherto been supposed.

(iii) The divergence between the results of this memoir and that of the former memoir on "Regression, Heredity, and Panmixia" would be fairly well accounted for, if there be a hitherto unobserved correlation between the hereditary influence and the fertility of woman.

"On the Magnetic Permeability of Liquid Oxygen and Liquid Air." By J. A. FLEMING, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London, and JAMES DEWAR, LL.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution, &c. Received November 20,—Read November 26, 1896.

The remarkable magnetic properties of liquid oxygen were pointed out by one of us in a communication to the Royal Society in 1891,*

* 'Roy. Soc. Proc.,' December 10th, 1891, vol. 51, p. 24. See a letter to the President by Professor James Dewar, F.R.S.